Lecture 1: Causal Inference and Potential Outcomes

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Road Map to Lecture 1

- *Potential outcomes and causal inference*
- *Average Treatment Effects (ATE)*
- Complier Average Causal Effect (CACE)
- *Intention to Treat Effect (ITT)*
- *Experimental Modes*

Defining treatment

- *The variable d_i indicates whether the ith subject is treated.*

- *In the typical case of binary treatments, $d_i = 1$ means the ith subject receives the treatment*

- *$d_i = 0$ means the ith subject does not receive the treatment.*

- *It is assumed that d_i is observed for every subject.*

Potential Outcomes

- Y_i : the potential outcome for subject *i*

- $Y_i\,(d_i\,)$: the outcome for subject *i*, written as a function of the treatment i received; it is generally the case that we observe only one of the potential outcomes for each *i*

- For the binary-valued treatment, there are two “potential outcomes”:

- Y_i (1), the potential outcome for i conditional on *i* being treated

- Y_i (0), the potential outcome for i conditional on *i* not being treated

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Potential Outcome Schedule

- "Hypothetical"
- *Comprehensive list of potential outcomes for all subjects*
- *r*ows of this schedule are indexed by *i*, and the columns are indexed by *d*
- *p*otential outcomes for the fifth subject may be found in adjacent columns of the fifth row

Potential Outcomes Local Budget

	Budget share if village head is male	Budget share if village head is female	Treatment Effect
Village 1	10	15	5
Village 2	15	15	0
Village 3	20	30	10
Village 4	20	15	-5
Village 5	10	20	10
Village 6	15	15	0
Village 7	15	30	15
Average	15	20	5

Potential Outcome Subgroup

- sometimes useful to refer to potential outcomes for a subset of the subjects
- expressions of the form $Y_i(d)|X=x$ denote potential outcomes when the condition $X=x$ holds.
- For example, $Y_i(0) \mid d_i=1$ refers to the untreated potential outcome for a subject who actually receives the treatment.

Individual Level Causal Effect

- For subject *i*, the effect of the treatment is conventionally defined as the difference between outcomes across the two potential outcomes:

$$\delta_i = Y_i(1) - Y_i(0)$$

- Alternatively:

$$Y_i = Y_i(0) + (Y_i(1) - Y_i(0))\, D_i$$

- Often referred to as the Rubin causal model; perhaps more appropriately, the Neyman-Holland-Rubin causal model;

- **The Fundamental Problem of Causal Inference** only one of the two potential outcomes is realized, so that δ_i is typically non- operational

Realized Potential Outcomes

- Use lower-case letters for realizations of the potential quantities (again, typically only one of the two potential outcomes is realized).

  1. y_i (1), the outcome observed for *i* conditional on d_i = 1 (*i* is treated)

  2. y_i (0), the outcome observed for *i* conditional on d_i = 0 (*i* is not treated)

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The Fundamental Problem of Causal Inference

Table 2.1, p35 Morgan and Winship, *Counterfactuals and Causal Inference*

Group	$Y_i(1)$	$Y_i(0)$
Treatment ($D_i = 1$)	**Observable**	Counterfactual
Control Group ($D_i = 0$)	Counterfactual	**Observable**

Observed Outcomes

- The connection between the observed outcome and the underlying potential outcomes is given by the equation $Y_i = d_i Y_i(1)+(1-d_i)Y_i(0)$.

- This equation indicates that the $Y_i(1)$ are observed for subjects who are treated, and the $Y_i(0)$ are observed for subjects who are not treated.

- For any given subject, we observe either $Y_i(1)$ or $Y_i(0)$, not both.

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Observed Outcomes Local Budget

	Budget share if village head is male	Budget share if village head is female
Village 1	?	15
Village 2	15	?
Village 3	20	?
Village 4	20	?
Village 5	10	?
Village 6	15	?
Village 7	?	30

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Treatment: Random Variable

- we often want to know about the statistical properties of a hypothetical random assignment

- hence we distinguish between d_i, the treatment that a given subject receives (a variable that one observes in an actual dataset), and D_i, the treatment that could be administered hypothetically.

- D_i is a random variable, and the ith subject might be treated in one hypothetical study and not in another.

- Yi(1) | Di=1 refers to the treated potential outcome for a subject who would be treated under some hypothetical allocation of treatments.

- We use capital whenever we talk about statistical properties (e.g., independence, expected value) of treatments.

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Average Treatment Effect

- Average Treatment Effect:
  $E(\delta) = E[Y(1)]–E[Y(0)] = E[Y(1)–Y(0)]$
- where the expectation is over a population, and so no subscript *i*.
- This is operational, in that we can compute sample estimates of E[Y(1)] and E[Y(0)]: e.g., the sample averages

$$\bar{y}(1) = \frac{1}{n_1} \sum_{i:d_i=1} y_i(1) \quad \text{and} \quad \bar{y}(0) = \frac{1}{n_0} \sum_{i:d_i=0} y_i(0)$$

- where n_1 and n_0 are the number of subjects in groups $d(1)$ and $d(0)$ respectively.

Randomization Generates Unbiased Estimates of Average Treatment Effect

- Rubin (1974) calls this:

$$\hat{\delta} = \bar{y}(1) - \bar{y}(0)$$

$$\bar{y}_d$$

- Under certain circumstances, this is an unbiased estimate of the population average treatment effect δ .

- Why? How?

- Nice, informal treatment in “Two Formal Benefits of Randomization”.

Unbiasedness?

- Remember that unbiasedness is an “on average” property
- “we tend to estimate the correct quantity, but it [randomization] hardly solves the problem of estimating the typical causal effect”
- Why? What is Rubin referring to? How can randomization “fail”?

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Naive Estimation

$$\underbrace{E(y_i|D_i=1)-E(y_i|D_i=0)}_{\text{Obs difference in avg } y} = \underbrace{E(Y_i(1)|D_i=1)-E(Y_i(0)|D_i=1)}_{\text{ATT: average treatment effect for the treated}}$$
$$+\underbrace{E(Y_i(0)|D_i=1)-E(Y_i(0)|D_i=0)}_{\text{selection bias}}$$

- Observed difference in average outcomes is often a biased estimator of an average treatment effect.

- n.b., selection bias is simply the difference between what the treated would have done had they not been treated and what the non-treated do when non-treated.

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Properties of Random Assignment?

- under equal probability random assignment, the conditional ATE among the treated is the same as the conditional ATE among the control group, which is therefore the same as the ATE.

- the expected $Y_i(0)$ in the treatment group is the same as the expected $Y_i(0)$ in the control group

- When random assignment is not used (i.e., observational research), the unbiasedness of the difference-in-means estimator becomes a matter of conjecture.

Conditional independence

- randomization ensures independence of potential outcomes and treatment (or assignment to treatment). conditional independence notation:
  $$(Y(1),Y(0)) \perp D$$

- When this holds
  - $E[Y_i(1)|D_i = 1] = E[Y_i(1)|D_i = 0] = E[Y_i(1)]$.
  - Similarly for $E[Y_i(0)]$.
  - $(Y(1), Y(0)) \perp D$, ATT can be rewritten as

$$\begin{aligned}
E(Y_i(1)|D_i = 1) - E(Y_i(0)|D_i = 1) \\
&= E(Y_i(1)|D_i = 1) - E(Y_i(0)|D_i = 0) \\
&= E(Y_i(1)|D_i = 1) - E(Y_i(0)|D_i = 1) \\
&= E(Y_i(1) - Y_i(0)) = \text{ATE}.
\end{aligned}$$

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Potential Outcomes: Core Assumptions

- We assume that each subject has two potential outcomes $Y_i(1)$ if treated and $Y_i(0)$ if not treated

- Each potential outcome depends **solely** on whether the subject **itself** receives the treatment

- Potential outcomes respond only to the treatment and not to some other feature of the experiment — such as assignment or measurement

Exclusion Restriction

- Let $Y_i(z, d)$ be the potential outcome when $z_i = z$ and $d_i = d$, for $z \in (0, 1)$ and for $d \in (0, 1)$
- For example, if $z_i = 1$ and $d_i = 1$, the subject is assigned to the treatment group and receives the treatment
- Or $z_i = 1$ and $d_i = 0$ – subject is assigned treatment but does not receive treatment
- The exclusion restriction is that $Y_i(1, d) = Y_i(0, d)$ – subjects only respond to input from d_i

- The excludability assumption cannot be verified empirically because we never observe both and for the same subject.

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Classic Drug Experiment Example

- treatment group receives a new drug
- control group receives nothing
- treatment = the active ingredients of the new drug
- experiment confounds this treatment with receipt of a pill
- If patients respond to the pill rather than the pill's ingredients, excludability is violated
- jeopardizes unbiasedness of the difference-in-means estimator

Non-interference

- Permits us to ignore the potential outcomes that would arise if subject *i* were affected by the treatment of other subjects

- Formally, we reduce the schedule of potential outcomes Yi(**d**), where **d** describes all of the treatments administered to all subjects, to a much simpler schedule Yi(d), where d refers to the treatment administered to subject *i*.

- implies that so long as a subject 's treatment status remains constant, that subject's outcome is unaffected by the particular way in which treatments happened to be assigned to other subjects.

Non-interference violated

- police patrols displace crime from treated to untreated areas
- non-interference violated if your estimand is following:
  - average potential outcome when a block is treated minus average potential outcome when no blocks are treated
- if police patrols displace crime from treated to untreated areas, observed outcomes in control will not be potential outcomes when no treatment administered anywhere
- estimated ATE will tend to exaggerate the true ATE.

Core assumptions violated?

- Blinding: Researchers are not blinded to experimental assignment when measuring outcomes

- Attrition: Some of the subjects in the treatment group become discouraged and drop out of the study

- Compensatory behavior: Upon noticing that some subjects are excluded from a poverty aid treatment because they were assigned to the control group, an aid organization endeavors to treat those who were assigned to the control group

- Multiple outcomes: A weight loss intervention randomly assigns students who come to a cafeteria for lunch to a treatment consisting of smaller dishes and portions; outcomes are measured in terms of total calories consumed at the cafeteria during lunchtime.

Average Treatment Effect (ATE)

$$ATE = \frac{1}{N}\sum_{i=1}^{N}(Y_i(D=1) - Y_i(D=0))$$

- i.e., treatment effects averaged over those *receiving treatment.*

Chattopadhyay & Duflo 2004

- Randomized policy experiment in India
- 1990s, one-third of village council heads reserved for women
- women.csv contains subset of data from West Bengal
- Gram Panchayat (GP) = level of government
- Analysis?
    - was randomization implemented properly?
    - Conjecture: more drinking facilities under women
    - Conjecture: no effect on irrigation

Name	Description
`GP`	An identifier for the Gram Panchayat (GP)
`village`	identifier for each village
`reserved`	binary variable indicating whether the GP was reserved for women leaders or not
`female`	binary variable indicating whether the GP had a female leader or not
`irrigation`	variable measuring the number of new or repaired irrigation facilities in the village since the reserve policy started
`water`	variable measuring the number of new or repaired drinking-water facilities in the village since the reserve policy started

Table 4.6: *The Variable Names and Descriptions of the Women as Policy Makers Data.*

```
> setwd("~/Dropbox/Experimental Methodology/DPIR
2017/qss-master/PREDICTION")
> women <- read.csv("women.csv")
> ##proportion of female politicians in reserved GP vs.
unreserved GP
> mean(women$female[women$reserved] == 1)
[1] 1
> mean(women$female[women$reserved == 0])
[1] 0.07476636
```

```r
## drinking-water facilities
mean(women$water[women$reserved == 1]) -
    mean(women$water[women$reserved == 0])

## [1] 9.252423

## irrigation facilities
mean(women$irrigation[women$reserved == 1]) -
    mean(women$irrigation[women$reserved == 0])

## [1] -0.3693319
```

Intent-to-treat effect

$$ITT = \frac{1}{N}\sum_{i=1}^{N}(Y_i(Z=1) - Y_i(Z=0))$$

- Where Z is an indicator for treatment assignment
- with 100% compliance ATE=ITT
- ITT captures the average effect of being assigned to the treatment group regardless of the proportion of the treatment group actually treated

Complier Average Causal Effect (CACE)

$$\text{CACE} = \frac{\text{ITT}}{\sigma}$$

- where σ is the share of those assigned to the treatment group receiving treatment
- CACE also referred to as Local Average Treatment Effect (LATE) and Treatment on Treated (TOT)
- ATE among Compliers

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ITT, ATE: Potential Outcomes

Obs	$Y_i(0)$	$Y_i(1)$	$D_i = 0$	$D_i = 1$	Type
1	4	6	0	1	Complier
2	2	8	0	0	Never-Taker
3	1	5	0	1	Complier
4	5	7	0	1	Complier
5	6	10	0	1	Complier
6	2	10	0	0	Never-Taker
7	6	9	0	1	Complier
8	2	5	0	1	Complier
9	5	9	0	0	Never-Taker

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Compare ATT, ATE, and CACE

- ATE does not consider noncompliance:

$$\text{ATE} = \frac{2+6+4+2+4+8+3+3+4}{9} = 4$$

- ITT accounts for the fact that never-takers will not receive the treatment:

$$\text{ITT} = \frac{2+0+4+2+4+0+3+3+0}{9} = 2$$

- CACE is based on the subset of Compilers:

$$\text{CACE} = \frac{2+4+2+4+3+3}{6} = 3$$

Personal Canvass & Voting

- Gerber and Green New Haven study APSR 2000
- Randomly assign voters different GOVT tactics
  - Personal canvassing contact?
  - Mail?
  - Telephone?
  - Control?

New Haven Voter Mobilization

Turnout Rate	Treatment Group	Control Group
Among those contacted	54.43 (395)	
Among those not contacted	36.48 (1050)	37.54 (5645)
Overall	41.38 (1445)	37.45 (5645)

- ITT = 41.38 – 37.54 = 3.84
- σ= 395/1445 =0.273
- CACE=ITT/σ= 3.84/0.273 = 14.1

Randomised Control Trials

- RCTs are conducted in a real-world setting (=field experiments)
  - Attempt to be unobtrusive
- They can minimise any artefacts of the experimental design - internal validity
  - e.g. experimenter demand effects in lab experiments
  - Make generalisations less dependent on assumptions

Randomised Control Trials

- Four main criteria
  - Treatment = real-world intervention?
  - Participants = those who would normally receive it?
  - Context = real-world context of interest?
  - Outcome measure = actual outcome of interest?

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Randomised Control Trials

- Four main criteria
  - Treatment = real-world intervention?
  - Participants = those who would normally receive it?
  - Context = real-world context of interest?
  - Outcome measure = actual outcome of interest?

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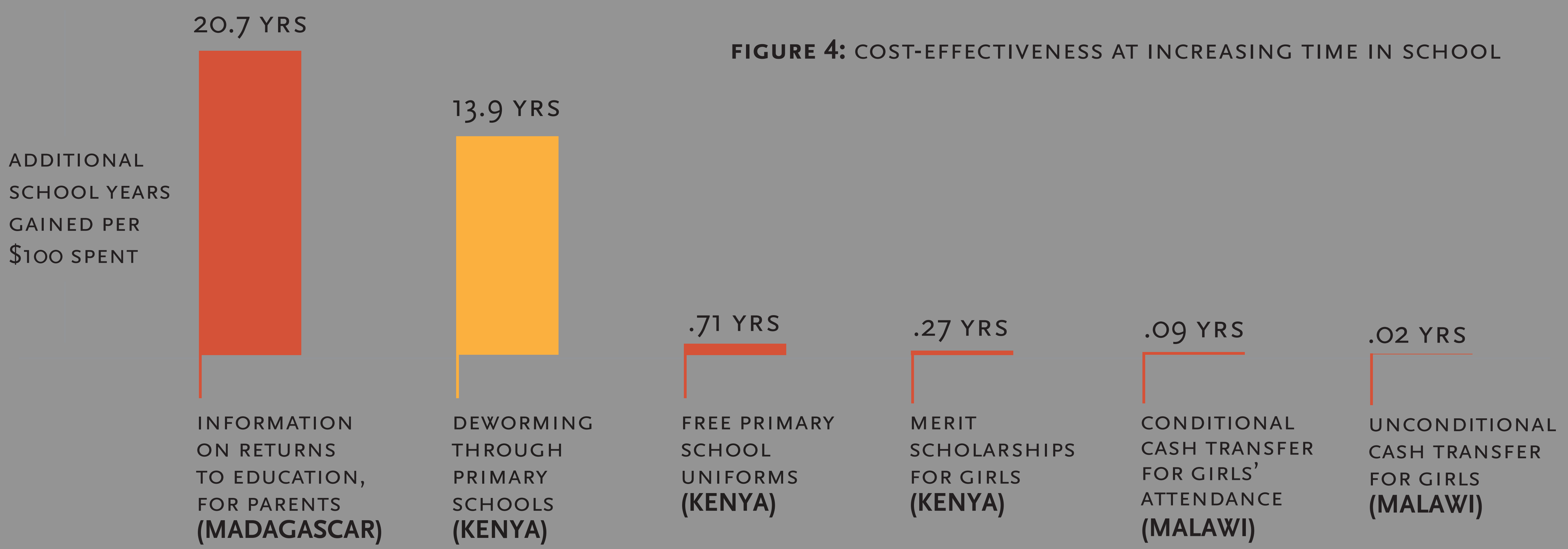
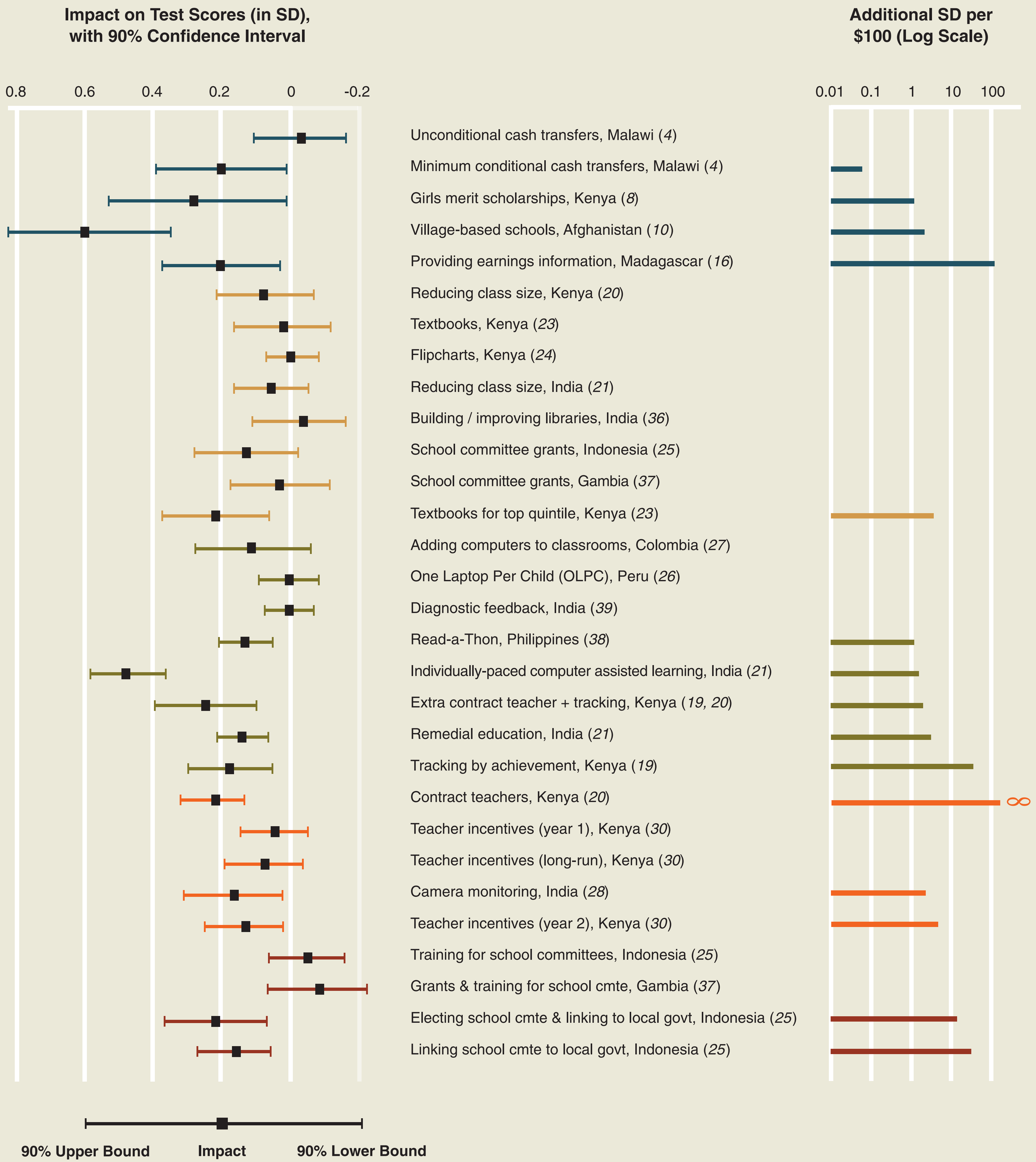
FIGURE 4: COST-EFFECTIVENESS AT INCREASING TIME IN SCHOOL




Impact on Test Scores (in SD), with 90% Confidence Interval
0.8
0.6
0.4
0.2
0
-0.2
Additional SD per $100 (Log Scale)
0.01
0.1
1
10
100
Unconditional cash transfers, Malawi (*4*)
Minimum conditional cash transfers, Malawi (*4*)
Girls merit scholarships, Kenya (*8*)
Village-based schools, Afghanistan (*10*)
Providing earnings information, Madagascar (*16*)
Reducing class size, Kenya (*20*)
Textbooks, Kenya (*23*)
Flipcharts, Kenya (*24*)
Reducing class size, India (*21*)
Building / improving libraries, India (*36*)
School committee grants, Indonesia (*25*)
School committee grants, Gambia (*37*)
Textbooks for top quintile, Kenya (*23*)
Adding computers to classrooms, Colombia (*27*)
One Laptop Per Child (OLPC), Peru (*26*)
Diagnostic feedback, India (*39*)
Read-a-Thon, Philippines (*38*)
Individually-paced computer assisted learning, India (*21*)
Extra contract teacher + tracking, Kenya (*19, 20*)
Remedial education, India (*21*)
Tracking by achievement, Kenya (*19*)
Contract teachers, Kenya (*20*)
∞
Teacher incentives (year 1), Kenya (*30*)
Teacher incentives (long-run), Kenya (*30*)
Camera monitoring, India (*28*)
Teacher incentives (year 2), Kenya (*30*)
Training for school committees, Indonesia (*25*)
Grants & training for school cmte, Gambia (*37*)
Electing school cmte & linking to local govt, Indonesia (*25*)
Linking school cmte to local govt, Indonesia (*25*)
90% Upper Bound
Impact
90% Lower Bound

Random Control Trial

- Bertrand and Mullainathan (2004)
- Does racial prejudice affect hiring of Blacks?
- How to control confounding factors such as educational attainment, etc.?
- Researchers control random assignment
- Random assignment of resumes to hiring employers

B & M: Race in the Labor Market

- Why is there racial inequality in the U.S. Labor Market?
- Is it the result of differential/discriminatory hiring treatment?
- Observational (read survey) data cannot answer this question because of unobservables -- survey data do not contain all the characteristics that employers observe
- Solution: a field experiment
- Solution: race randomly assigned to fictitious resume
- Solution: paper resumes eliminate demand effects

B & M: Design

- Collect resumes from web sites; classify into Low and High quality; augment
- Identify distinctive White and Black names
- Four resumes selected for each advertised job -- High/Low to White; High/Low to Black
- 1,300 ads -- 5,000 resumes sent
- Measured response: Employer callback or e-mail

```
> setwd("~/Dropbox/Experimental Methodology/DPIR 2017/
qss-master/CAUSALITY")
> read.csv("resume.csv")
      firstname    sex  race call
1       Allison female white    0
2       Kristen female white    0
3       Lakisha female black    0
4       Latonya female black    0
5        Carrie female white    0
6           Jay   male white    0
7          Jill female white    0
8         Kenya female black    0
9       Latonya female black    0
10       Tyrone   male black    0
11        Aisha female black    0
12      Allison female white    0
```

```
> setwd("~/Dropbox/Experimental Methodology/DPIR 2017/qss-master/
CAUSALITY")
> india <-  read.csv("resume.csv")
> summary(india)
   firstname          sex             race              call        
 Tamika : 256   female:3746   black:2435   Min.   :0.00000  
 Anne   : 242   male  :1124   white:2435   1st Qu.:0.00000  
 Allison: 232                              Median :0.00000  
 Latonya: 230                              Mean   :0.08049  
 Emily  : 227                              3rd Qu.:0.00000  
 Latoya : 226                              Max.   :1.00000  
 (Other):3457                                               
```

```
> ## overall callback rate: total callbacks divided by the sample size
> sum(race.call.tab[,2])/nrow(resume)
[1] 0.08049281
> ## callback rates for each race
> race.call.tab[1,2] / sum(race.call.tab [1, ]) # black
[1] 0.06447639
> race.call.tab[2,2] / sum(race.call.tab [2, ]) # white
[1] 0.09650924
```

B & M: Results

- White names have 9.65 percent chance of receiving call back
- African-American names have a 6.45 percent chance of call back
- 1 in 10 for Whites -- 1 in 15 for Blacks
- Return to White name equals about 8 additional years of experience
- Whites favored by 8.4 percent compared to 3.5 employers favoring Blacks

TABLE 1—MEAN CALLBACK RATES BY RACIAL SOUNDINGNESS OF NAMES

	Percent callback for White names	Percent callback for African-American names	Ratio	Percent difference (*p*-value)
Sample:				
All sent resumes	9.65	6.45	1.50	3.20
	[2,435]	[2,435]		(0.0000)
Chicago	8.06	5.40	1.49	2.66
	[1,352]	[1,352]		(0.0057)
Boston	11.63	7.76	1.50	4.05
	[1,083]	[1,083]		(0.0023)
Females	9.89	6.63	1.49	3.26
	[1,860]	[1,886]		(0.0003)
Females in administrative jobs	10.46	6.55	1.60	3.91
	[1,358]	[1,359]		(0.0003)
Females in sales jobs	8.37	6.83	1.22	1.54
	[502]	[527]		(0.3523)
Males	8.87	5.83	1.52	3.04
	[575]	[549]		(0.0513)

TABLE 3—RESUME CHARACTERISTICS: SUMMARY STATISTICS

Sample:	All resumes	White names	African-American	Higher quality	Lower quality
Characteristic:					
College degree	0.72	0.72	0.72	0.72	0.71
(Y = 1)	(0.45)	(0.45)	(0.45)	(0.45)	(0.45)
Years of experience	7.84	7.86	7.83	8.29	7.39
	(5.04)	(5.07)	(5.01)	(5.29)	(4.75)
Volunteering experience?	0.41	0.41	0.41	0.79	0.03
(Y = 1)	(0.49)	(0.49)	(0.49)	(0.41)	(0.16)
Military experience?	0.10	0.09	0.10	0.19	0.00
(Y = 1)	(0.30)	(0.29)	(0.30)	(0.39)	(0.06)
E-mail address?	0.48	0.48	0.48	0.92	0.03
(Y = 1)	(0.50)	(0.50)	(0.50)	(0.27)	(0.17)
Employment holes?	0.45	0.45	0.45	0.34	0.56
(Y = 1)	(0.50)	(0.50)	(0.50)	(0.47)	(0.50)
Work in school?	0.56	0.56	0.56	0.72	0.40
(Y = 1)	(0.50)	(0.50)	(0.50)	(0.45)	(0.49)
Honors?	0.05	0.05	0.05	0.07	0.03
(Y = 1)	(0.22)	(0.23)	(0.22)	(0.25)	(0.18)
Computer skills?	0.82	0.81	0.83	0.91	0.73
(Y = 1)	(0.38)	(0.39)	(0.37)	(0.29)	(0.44)
Special skills?	0.33	0.33	0.33	0.36	0.30
(Y = 1)	(0.47)	(0.47)	(0.47)	(0.48)	(0.46)
Fraction high school dropouts in	0.19	0.19	0.19	0.19	0.18
applicant's zip code	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Fraction college or more in	0.21	0.21	0.21	0.21	0.22
applicant's zip code	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)
Fraction Whites in applicant's zip	0.54	0.55	0.54	0.53	0.55
code	(0.33)	(0.33)	(0.33)	(0.33)	(0.33)
Fraction African-Americans in	0.31	0.31	0.31	0.32	0.31
applicant's zip code	(0.33)	(0.33)	(0.33)	(0.33)	(0.33)
Log(median per capital income)	9.55	9.55	9.55	9.54	9.56
in applicant's zip code	(0.56)	(0.56)	(0.55)	(0.54)	(0.57)
Sample size	4,870	2,435	2,435	2,446	2,424

Table 4—Average Callback Rates By Racial Soundingness of Names and Resume Quality

Panel A: Subjective Measure of Quality
(Percent Callback)

	Low	High	Ratio	Difference (*p*-value)
White names	8.50	10.79	1.27	2.29
	[1,212]	[1,223]		(0.0557)
African-American names	6.19	6.70	1.08	0.51
	[1,212]	[1,223]		(0.6084)

Panel B: Predicted Measure of Quality
(Percent Callback)

	Low	High	Ratio	Difference (*p*- value)
White names	7.18	13.60	1.89	6.42
	[822]	[816]		(0.0000)
African-American names	5.37	8.60	1.60	3.23
	[819]	[814]		(0.0104)

Potential Outcomes Framework

CV	Black-sounding	Callback			
I	Name Ti	Yi(1)	Yi(0)	Age	Education
1	1	1	?	20	College
2	0	?	0	55	H School
3	0	?	1	40	G School
.	.	.	.	.	.
.	.	.	.	.	.
.	.	.	.	.	.
n	1	0	?	62	College

Social Pressure & Voting

- Social pressure within neighborhood increases vote turnout?
- Randomly assign voters different GOVT messages
  - Did these postcards increase vote turnout?
  - Naming-and-shaming GOVT message?
  - Hawthorne effect

Turnout Data

```
> social <- read.csv("social.csv") #load the data
> summary(social) #summarize the data
     sex          yearofbirth    primary2004            messages
 female:152702   Min.   :1900   Min.   :0.0000   Civic Duty: 38218
 male  :153164   1st Qu.:1947   1st Qu.:0.0000   Control   :191243
                 Median :1956   Median :0.0000   Hawthorne : 38204
                 Mean   :1956   Mean   :0.4014   Neighbors : 38201
                 3rd Qu.:1965   3rd Qu.:1.0000
                 Max.   :1986   Max.   :1.0000
  primary2006         hhsize
 Min.   :0.0000   Min.   :1.000
 1st Qu.:0.0000   1st Qu.:2.000
 Median :0.0000   Median :2.000
 Mean   :0.3122   Mean   :2.184
 3rd Qu.:1.0000   3rd Qu.:2.000
 Max.   :1.0000   Max.   :8.000
```

Name	Description
`hhsize`	household size of voter
`messages`	GOTV messages voter received (`Civic`, `Control`, `Neighbors`, `Hawthorne`)
`sex`	sex of voter (`female` or `male`)
`yearofbirth`	year of birth of voter
`primary2004`	whether a voter turned out in the 2004 Primary election (`1`=voted, `0`=abstained)
`primary2008`	whether a voter turned out in the 2008 Primary election (`1`=voted, `0`=abstained)

Table 2.3: *The Names and Descriptions of Variables in the Social Pressure Experiment Data.*

Treatment Effects

```
> ## turnout for each group
> tapply(social$primary2006, social$messages, mean)
Civic Duty    Control  Hawthorne  Neighbors
 0.3145377  0.2966383  0.3223746  0.3779482

> ## subtract control group turnout for each group
> tapply(social$primary2006, social$messages, mean) -
mean(social$primary2006[social$messages =="Control"])
Civic Duty    Control  Hawthorne  Neighbors
0.01789934 0.00000000 0.02573631 0.08130991
```

Balance

```
> social$age <- 2006 - social$yearofbirth # create age variable
> tapply(social$age, social$messages, mean)
Civic Duty    Control  Hawthorne  Neighbors
  49.65904   49.81355   49.70480   49.85294
> tapply(social$primary2004, social$messages, mean)
Civic Duty    Control  Hawthorne  Neighbors
 0.3994453  0.4003388  0.4032300  0.4066647
> tapply(social$hhsize, social$messages, mean)
Civic Duty    Control  Hawthorne  Neighbors
  2.189126   2.183667   2.180138   2.187770
```